Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Wim J. Kimmerer

Final Selection Panel Review

Proposal Title

#0107: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Funding:

Fund in part
Amount: **$1,170,000**

The final Selection Panel concurred with its initial findings on this proposal and recommended funding the proposal at the reduced amount recommended as a result of those deliberations. Should the California Bay−Delta Authority accept the Selection Panel's recommendation and approve the funding of this proposal, the applicant will be allowed to negotiate which tasks and associated costs will be reduced as part of the contracting process.

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0107: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Funding:

Fund in part
Amount: **$1,170,000**

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Relative Stresses On Key Fish Species
- Delta Smelt−related Projects

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

This proposal does not focus on Delta smelt or other delta fishes per se. In fact it addresses an issue more fundamental and as important to understanding the survival and production of fishes in the Delta. They propose a collaborative program of field and laboratory research focused on the lower trophic levels in the low salinity zone of the Delta and Suisan Bay. They hope to better understand the source and fate of the primary and secondary productivity supporting Delta fishes and how changes in the foodchain may have precipitated significant changes in energy flow and secondary production of the species and trophic levels that are a key to the recruitment and survival of at least some of the Delta fishes, including the endangered Delta smelt. This goal has significant strategic importance for management objective and may be a key to

understanding the low fish production within the system.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

Although the budget is large (1.3M) it seems fairly reasonable for the number of personnel and effort involved. Most of the budget is going toward salaries of a fairly large and diverse research team. The field studies will be completed in years 1 and 2 and the effort of each team was cut to about half in the third year when they have proposed to focus on a synthesis of the results from year 1 and 2 field and lab studies. Given some of the concerns expressed by the PI concerning scheduling of research and the contracting of the funding this is a reasonable approach that should assure refined and well synthesized results.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

Although some concerns were raised about the responsibilities for project management by the technical panel, I believe it is clear from the proposal and the effort requested in the budget that Kimmerer will provide the leadership for the project management and reporting. The strategy for relating the study results to Delta smelt and other LSZ fishes is weak in the proposal. However, Kimmerer and his previous and proposed studies and experience put him and his multiple collaborative teams in a good position to provide a comprehensive synthesis of food and flow effects on fishes and their foodchain. Kimmerer is also in an excellent position to identify holes in understanding of estuarine to be filled by future research. This project may not provide all the answers to the issue of how the changes in the foodchain and productivity of the system have affected Delta smelt and other declining Delta fishes but it is a critical step along the path to

understanding obvious changes in the system.

Selection Panel (Discussion) Review

fund this amount: $**1,170,000**
note:
fund in part

This proposal seeks to understand food web dynamics in the low−salinity zone of the Estuary (where Delta smelt spend much of their life) to examine the hypothesis that Delta smelt populations are limited by their food supply. This is an important topic for smelt and other native fishes that must be addressed. Also, this project will greatly increase the value of the products from another project (#106), which is proposed by some of the same members of this highly qualified work team.

Previous reviews raised questions about project management. The Panel strongly encourages the proponents to address those comments as they develop a final work plan. Also, previous reviews questioned the decision not to analyze the nano−plankton fraction of the zooplankton community. The Panel agreed with those comments and recommend funding only if the proponents devise a plan to conduct analyses of nano−plankton as part of this project, with no increase in budget

The proponents should collaborate closely with other relevant monitoring and modeling efforts related to the topics of food web dynamics, productivity, and fish abundance and distribution in the Delta. For example, the panel anticipates that there will be additional or augmented monitoring programs that come on−line between now and the time work begins – this project team should (and will likely) help with the development of this sampling so that it supports research questions like theirs.

The Panel felt the overall budget could be reduced by approximately 10% given that there is an entire year dedicated to “synthesis”.

Panel Ranking: Fund with Modifications

Collaboration Panel Review

Proposal Title

#0107: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Final Panel Rating
above average

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

superior
There are five inter−related components of this proposal (phytoplankton dynamics, benthic biomass and grazing, response of bacterial production to inputs, microbial foodweb, and copepod production) designed to tell a coherent story about the foodweb of delta smelt. The proposal also addresses the basis for the relationship between abundance of estuarine fish species and freshwater flow. The outcome will be a foodweb model with quantitative functional relationships among components, and estimates of likely response of the foodweb to flow. In this way, the subprojects are designed to be part of a larger collaborative proposal.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

above average
There is a conceptual model in the proposal that depicts the foodweb and various components of it and demonstrates the links between the various project components. In addition, the proposal provides a project organization diagram (p. 22) that outlines hwo the project components fit together. In addition, the proposal articulates plans for synthesis of the results of the various subprojects. The project team intends to meet at the inception of the project and begin this collaboration with a meeting to jointly analyze existing, relevant data. In addition, project team meetings are planned for twice a year, and a final synthesis meeting is planned. These plans are reflected in the budget.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

adequate
The lead PI (Kimmerer) "will be responsible for gathering information and developing semi-annual progress reports, for liaison with contracting personnel, and for ensuring coordination among tasks." Resources have been set aside for travel for collaboration, but there is not a management task, nor associated management budget. There is no acknowledgement of potential barriers to collaboration and strategies of overcoming them.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

above average
It appears as if the PI has successful management experience leading collaborative teams based on past funding, but this

skill is not explicitly stated. The team members appear to have complementary skills based on a review of CVs, but these complementarities are not described explicitly in the proposal. Based on proposal tasks, it appears as if the key personnel have been identified and are ready to proceed with implementation.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

adequate
There is a plan for reporting to CALFED via progress reports, including a synthesis report, and presentations to the Science Program and at the Science Conference. Outreach is mentioned, but it has not been explicitly tasked or budgeted.

Additional Comments:

Collaboration Panel (Discussion) Review

There is solid collaboration in this proposal that integrates 5 components. The sub-projects are designed to be worked into the overall project; a project organization diagram was provided that makes it clear how the sub-projects are linked. The synthesis of sub-project results is discussed. The team has planned a group meeting to analyze existing data. The Project management component is adequate. There is funding set aside for travel, but no associated management tasks. The PI has successful experience leading collaborative teams but it is not explicitly stated how he will manage this group. Applicant skills seem complementary but it is not specifically described. The dissemination products include a synthesis report and science program presentation; outreach is discussed but not budgeted.

Technical Synthesis Panel Review

Proposal Title

#0107: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Final Panel Rating
above average

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Authors clearly want to understand the food relations of this oligohaline species relative to an array of other variables. The general idea is that this species is ultrasensitive to what is happening in the upper estuary. The approach may be overly simplistic in that this one species may be affected more by the presence of an introduced clam. Nevertheless, the goal is clear. Some very good science will result from this proposed study. The only question is whether all of the goals will be met and all hypotheses answered. That will be difficult given the nature of this type of work and this species.

Additional Comments:

The PIs employ the method best suited to answer each hypothesis, and these are varied. Some of the methods are well−tried and relatively simplistic, but they work. Despite the varied methods applied to each question, PIs have thought about the end result and how to put this all together. Products are peer reviewed papers and answers to some fundamental questions regarding this species. There should be some attempt to put this research into formats that both the lay public and regulators can access. It often takes several

years for a paper to be published and even longer for overworked agency staff to find this information.

Authors clearly want to understand the food relations of this oligohaline species relative to an array of other variables. The general idea is that this species is ultrasensitive to what is happening in the upper estuary. The approach may be overly simplistic in that this one species may be affected more by the presence of an introduced clam. Nevertheless, the goal is clear. Some very good science will result from this proposed study. The only question is whether all of the goals will be met and all hypotheses answered. That will be difficult given the nature of this type of work and this species.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The technical reviewers and the primary and secondary panel members for this proposal were in agreement that this was a very good proposal without significant concerns, and that the team has the capability to perform the study. It was noted that this is a species of interest that is poorly understood. The proposed study could be improved by providing a more detailed description of how they propose the data will be synthesized, how the resulting data will improve our understanding of Delta smelt (and of estuarine species in general), and how the results of this study would be used by management agencies. There is a slight inconsistency with the external reviews in that the reviewer who gave it an overall "good" rating, gave "excellent" ratings for many of the individual categories. Much of the criticism was for inadequate detail on methods; possibly over emphasized.

Technical Review #1

proposal title: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**In the Executive Summary, the PIs identify two critical CALFED issues, food web support for the delta smelt and the basis for the relationships between abundance of estuarine fish species and freshwater flow, as the primary rationales for the proposed research. The proposal outlines a three−year field and laboratory research program that would focus on assessing phytoplankton, zooplankton and bacterial processes in relation to environmental and hydrological characteristics of the low−salinity zone (LSZ) of the estuary on the premise that these controls on the base of the food web regulate the potential production of the delta smelt.** **In general terms, these goals are clear and in a textbook sense are linked. As detailed later, however, a weakness of the proposal is that the individual research goals (e.g. phytoplankton, zooplankton and other studies) are not as clearly linked back to the overall stated goals of the project as they could be.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The PIs provide a clear conceptual model and excellent background information on the loss of phytoplankton biomass and production as a result of introduced benthic filter feeders. In contrast, the lengthy discussion of the 'X2' relationship, which should convince the reader of hydrologic regulation, is not clearly applied to the question at hand in this proposal. It was disappointing that there is no presentation of preliminary data on DOC/DOM concentrations in the LSZ as the microbial loop and bacteria relationships with hydrologic forcing are a critical aspect of the proposal.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Each of the individual tasks (e.g. phytoplankton, bacteria, zooplankton, etc…) is important to the questions being asked and the general methods presented are appropriate. These tasks by themselves will add to knowledge of the LSZ region. I feel, however, that there are two major weaknesses to the approach as it is presented:** **(1) The synthesis component of the project (Pg19) is extremely weak for a project of this magnitude. My sense is that there needs to be an independent task/effort/leader for this component and that it**

	needs to bring a significant modeling element added. (2) There is no effort to directly link the primary tasks back to the question of the effect of these food web processes on delta smelt and other fishes. **As it stands, the proposal has the potential to generate interesting data on several key components of the food web, but it is difficult to see how this will help management decisions without additional (and significant) addition effort. [NOTE: the PI listed two other separate CALFED projects that may address these issues if funded, but the lack of integration into this proposal is a weakness in my mind.]**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The individual tasks are accepted methods, adequately documented and technically feasible. The individual components are likely to succeed, however, as noted above, direct connection to the delta smelt (as suggested and advertised as a goal) and full integration are lacking. It is not clear that any of the individual PIs has the background to carry out the full integration (modeling?).**
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**N/A**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Primary literature publications are seen as major products of this effort. The previous track record of the individual PIs suggests that these products will be a priority and will be successful. There is no discussion/commitment to working with the management community to integrate these results into management practice.**
Rating	**very good**

Additional Comments

Comments	**N/A**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The capabilities of the PIs are excellent (as individual projects) and the support infrastructure is very good.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Yes, although specific duties of various support personnel are not detailed and add significantly to the overall cost of the project. My sense is that the proposed field/laboratory research and**

	synthesis/modeling should be able to be done within the basic budget.
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**There are much good science outlined in this proposal, but the lack of direct connection back to the delta smelt and limited synthesis weakens the proposal.**
Rating	**very good**

Technical Review #2

proposal title: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The objectives of the proposed research are stated clearly, as questions, on page 1 of the proposal, and rephrased as hypotheses on page 12. They are internally consistent overall. The questions are timely in that they address food web conditions supporting understanding of the population ecology of delta smelt, a threatened species in the study region.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**An extensive scientific background is summarized succinctly, and a conceptual model is presented (page 2). The model (Figure 1) summarizes most of the basis for the proposed work . Although the role of POC in the target food web is discussed in various parts of the text, POC does not appear explicitly in the model. The selection of the actual research topics is well justified.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The proposed project consists of 5 components (listed on page 12). Overall, the methods are standard, and nothing truly novel is proposed. The location of the study and the particular foodweb under study are unusual. The methodological approaches for each of the components are presented with varying degrees of detail and justification.** **The field plan is to conduct weekly cruises in spring to 2 stations defined by salinity regime, and then to sample every 3 weeks through late summer, for a total of ca. 12 cruises in each of 2 years. The definition of sampling sites based on salinity is an excellent approach. I question whether weekly sampling is truly adequate to fully capture the spin−up, peak, and spin−down of the spring bloom.** **The phytoplankton studies are described and justified in a great deal of detail. The methods for nutrient and phytoplankton standing stocks, nutrient uptake, and14−C primary production are standard and the best available. The enclosure experiments to study the interaction of light, nutrients and salinity on phytoplankton growth are interesting, but the enclosure volume seems to be on the small side (3.8L) for 5−day incubations.** **Methods to measure benthic biomass appear to be standard. I am not qualified to evaluate the section on determination of benthic production. It appears that the benthic component does not propose to measure grazing by clams directly, but estimates this parameter by some sort of back−calculation that is not described in the methods section of the proposal (a**

paper in press by Thompson is cited).

The section on bacterial methods is quite thin. The proposed methods seem to be standard. However, the reader has no idea how many measurements will be made, at what depths in the water column, etc. Although the importance of particulate material in this ecosystem is noted repeatedly in the sections on scientific background, the microbial food web and zooplankton, the section on bacterial methods does not mention particle−associated bacteria. It is not clear to me whether the bacterial component is restricted to free−living bacteria or includes bacteria associated with particles.

The section on the microbial foodweb is also rather thin and lacks detail. Target water column depths for the experiments are not specified. The focus is on herbivory and bacterivory by microzooplankton, which the authors' define as 20−200 um in size. This size range excludes the nanozooplankton (2−20 um), which I would expect to perform a significant amount of the bacterivory. Their grazing activity on both chlorophyll and FLB will be included in the dilution experiments. There is no mention of determining nanozooplanton vertical distribution, abundance or biomass−−to do so, a separate set of samples must be collected, preserved, and processed appropriately for analysis by epifluorescence microscopy. One would want this information in order to perform complete calculations of herbivory from standing stocks and literature values of grazing rates, as proposed. The authors state that microzooplankton abundance and species composition will be determined, but do not provide any information about the depths to be sampled, preservation methods, etc. Similarly, no detail is provided about how the seawater dilution technique will be applied in what is emphatically a turbid, low−light environment. When using chlorophyll as a proxy for phytoplankton in such a system, simulation of the in situ light environment is

	critical. Other important details, such as nutrient additions to the incubation bottles, are not mentioned. **The methods section on copepod production is well articulated. However, I would note that the same concern stated above about controlling for effects of nutrient enhancement in experimental treatments applies here: unless excess ammonia is added to control treatments, trophic cascade effects may obscure grazing information.** **Taken in total, the results of the proposed studies are likely to add to the base of knowledge about turbid estuaries. Provided that methodological details are attended to, the project will generate solid information about a specific portion of a specific estuary that should be able to be extrapolated to other turbid estuaries.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is not fully documented, as noted above. However, based on the PIs experience and expertise, I would say that the project is technically feasible overall, and the likelihood of success is high. The scale of the project is ambitious, but doable.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Not applicable.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**A number of peer-reviewed publications, including a synthesis paper, are likely to arise from the project. The data will be submitted to a web-based database (IEP) within 1 year of manuscript completion. I suggest that data submission as soon as quality-controlled data are available is more appropriate.**
Rating	**excellent**

Additional Comments

Comments	**The proposal includes references to 2 models: (1) the food web model (Fig. 1) and (2) the TRIM-3D model. It is not stated how the models will be run, and who will have responsibility for running them. The proposal package would be strengthened considerably by inclusion of a named modeler with specific expertise and responsibility.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**All of the PIs possess recognized expertise, and all have excellent publication records. Those of Wilkerson, Dugdale, Carpenter and Kimmerer are outstanding. The infrastructure specified on pp. 19-20 is adequate to support the project.**

Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget seems reasonable for the work proposed.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**Because of the lack of information abot some of the methods, I rate this proposal good/very good overall. Had more complete information been propvided, I would probably have rated it very good/excellent.**
Rating	**good**

Technical Review #3

proposal title: Foodweb support for the threatened delta smelt and other estuarine fishes in Suisun Bay and the western Sacramento−San Joaquin Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The overall goal of this study is to understand the trophic dynamics of the oligohaline zone of this California estuary and how this affects the declining populations of a threatened fish species. The delta smelt is being used as an indicator species for effects of freshwater inflows, and this foodweb approach should provide the background information to allow an understanding of the effects of freshwater inflow on productivity and species diversity in the oligohaline zone. The goals are clearly stated and backed with clear scientific documentation. This is an important areas of research for California and the rest of the country as demand for freshwater resources continue to grow and the impacts of freshwater on estuarine systems are better understood.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This study clearly states the case for filling critical areas that are lacking in our present understanding of the role of freshwater input to**

	estuarine productivity and species diversity. The conceptual food web model is clearly presented, and within accepted paradigms for marine and estuarine ecosystems. This level of study is well justified in this proposal.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach proposed uses standard methods that are all well accepted in the field of aquatic ecology to study the various trophic levels from primary producers to zooplankton. The addition to the base of knowledge will be more of an incremental nature rather than being truly novel. It was not clear to me exactly how this information would be used by decision makers regarding freshwater inflows. These mechanisms may exist, but they were not clearly laid out in the proposal.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach and methods are carefully documented and well accepted methods in aquatic science. All methods have been successfully used in other studies. The scale of the project seems consistent with the objectives the authors have laid out.**
Rating	

	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The plan to monitor planktonic organisms based on a salinity zone rather than fixed stations is the appropriate approach for studying an oligohaline zone that may shift up and down an estuary with variations in freshwater inflow. Alternately, benthic organisms are correctly being monitored from fixed locations.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The main products from this project will be top−quality, peer−reviewed scientific publications that will further our understanding of estuarine food webs and their role in management of freshwater inflows and threatened species.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The principal investigators in this project are all internationally known and well-respected scientists with excellent publication records. They are clearly capable of carrying out the proposed research.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The overall budget may seem high, but it is reasonable considered the scope of the work proposed. Ecosystem research is expensive, but this group can deliver what they promise for the money. I think it would be money well spent.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**There have been relatively few studies of the effects of freshwater inflow on oligohaline estuarine food webs. The inclusion of the contribution of DOM (including the possible lysis of freshwater phytoplankton) and bacterial production to the food web takes this study beyond the standard estuarine ecosystem study. The PI's are all well known scientists with excellent track records. This project has a very high probability for success. I would give this project a very high priority for funding.**
Rating	**excellent**